account of being illustrated by thirty coloured plates, which are excellent examples of the three-colour process. Not only are they for the most part good pictures of the animals they purport to represent, but they also show in most cases the natural surroundings of the respective species, so far as these can be reproduced with fidelity to nature. Unfortunately, in one instance that has come under our notice the wrong species has been depicted. We refer to the plate of giraffes and zebras facing p. 42, where the animal purporting to be the true or mountain zebra (*Equus zebra*) is the northern race of the bonte-quagga, or Burchell's zebra (*Equus burchelli granti*). Had the true zebra been depicted, the plate would then have been wrong, as showing a mountain animal on the open plains. The text includes a brief but well-compiled survey of the whole animal kingdom, from Primates to Protozoa, including sporatozoans, with some mention of extinct forms, and the work as a whole appears thoroughly suited to its purpose.

R. L.

Précis des Caractères génériques des Insectes, disposés dans un Ordre naturel par le Citoyen Latreille. A Paris, chez Prévôt, Libraire, Quai des Augustins, et à Brive, chez F. Bourdeaux, Imprimeur Libraire. A Brive, de l'Imprimérie de F. Bourdeaux, an 5 de la R. [1796]. Pp. xiv+208. (Imprime à 200 Exemplaires par A. Hermann, n.d.) Price 7 francs.

THIS is a reprint of a very rare book, which is seldom to be found even in the best entomological libraries. It commences with a table of the fourteen classes into which the author divides insects (understanding by that term Arachnida, Crustacea, and Myriopoda, as well as Hexapoda), followed by systematic characters for 351 genera recognised by Latreille, one or two others, described by other authors, being passed over as at present unknown to him. Several very familiar genera, such as Ypsolophus and Adela, are characterised in this book for the first time, but without indication of types, for which reference must be made to Latreille's later works. It is always unsatisfactory to entomological authors to be in doubt respecting the actual contents of a book they only know at second-hand, and we are glad that this scarce book has now been rendered accessible to them in a complete (and, we hope, accurate) reprint.

The Metric and British Systems of Weights, Measures and Coinage. By Dr. F. Mollwo Perkin. Pp. 83; with 17 diagrams. (London: Whittaker and Co., 1907.) Price 1s. 6*d*.

DR. PERKIN here provides a simply written and clearly expressed account of metric measures of length, area, volume and weight, and, in addition, treats of specific gravities, temperature measurements, and money. As Lord Kelvin has said, "our weights and measures are time-wasting and brain-wearing," and all attempts to familiarise British boys with the simplicity and convenience of the decimal system deserve encouragement. This small volume should prove useful in both day and evening schools, and all engaged in manufacture and commerce would derive advantage from its study.

The Story of Scraggles (A Sparrow). By George W. James. Illustrated from drawings by Sears Gallagher and from photographs. Pp. 88. (London: Chatto and Windus, 1907.) Price 2s. 6*d*.

THE greater part of this booklet is in the form of the autobiography of an ailing song-sparrow, which could not fly, and was kept in a house for three months. The story, which purports to represent the bird's thoughts and feelings, will probably appeal to little girls, and encourage them to be kind to animals.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Specific Stability and Mutation.

MR. R. H. LOCK at the close of his letter in NATURE of October 17 (vol. lxxvi., p. 616) makes a remark which, after some consideration, still perplexes me. The question involved is so interesting and, indeed, important, that I feel sure that many of your readers would be glad to know the grounds, doubtless not without weight, which led him to it. I quote the passage:—"that natural conditions lead to the obliteration of a host of mutations is as fair a deduction from the fact that such mutations appear under cultivation as the current deduction that the conditions of cultivation actually cause the occurrence of this kind of variation. We have the testimony of de Vries and others that the former process actually takes place. That the latter process does so is an assumption which still lacks the support of facts."

It contains two propositions:—(1) mutations appear under cultivation; (2) that the conditions of cultivation actually cause the occurrence of this kind of variation . . . is an assumption which still lacks the support of facts. It is the latter on which I think some discussion would probably be illuminating. At first sight the two propositions look contradictory. It is possible, however, that Mr. Lock is using "cause" in a very technical sense. But as Mill remarks, "in practice that particular condition is usually styled the cause, whose share in the matter is superficially the most conspicuous." Now, our knowledge of mutations is almost exclusively drawn from cultivated plants. In such cases cultural conditions are obviously an inseparable antecedent to mutations. What I do not understand is why it is an "assumption" to state that there is a causal nexus between the effect and the conditions.

To avoid ambiguity, I may explain that by "variation" I mean insensible and continuous organic change; by "mutation," that which is large, palpable, and discontinuous.

I believe that in nature variation never ceases, and yet "species" do not perceptibly vary. I pointed out some years ago, in a discussion at the Royal Society, the explanation of this seeming paradox. What we mean by a species is an abstraction which has no concrete existence. It is the mean or average of a host of varying individuals. It will be hardly contested nowadays that, so long as the conditions remain constant, the species remains unchanged. As has been frequently pointed out, the Egyptian flora furnishes a remarkable illustration of the fact over a long period of time. Plant remains from tombs believed to be 4000 years old differ in no respect from the species now living to which they belong, and the mere fact of their preservation shows that the physical conditions have undergone no change. To this persistence under constant surroundings I have given the name of "specific stability."

That mutations occur and exist is obvious to everyone. But that they are of frequent occurrence under purely natural conditions is, I think, unsupported by evidence, and, if they do occur, I agree with Darwin that it may be doubted if they "are ever permanently propagated in a state of nature" ("Origin," fifth edition, p. 49). The reason is that an organism is so nicely adjusted to its surroundings that it is in the highest degree improbable that a sudden and extreme structural change would fit in with them.

If species arise in nature by discontinuous variation or mutation, one would expect to see some evidence of their doing so; but in the British flora I can only call to mind a very few instances. There is a form of *Chelidonium majus* with laciniate leaves and petals. It is said to have first appeared at Heidelberg in 1590; according to Hooker it is only known in cultivation. There are two striking mutations of *Plantago major*, one with a paniculate inflorescence, the other with leafy bracts, which reappear from time to time independently, but always, I think, in cultivated ground. There is a tendency in maples to revert to a ternary symmetry, as is shown by the occasional production of three carpels instead of two. The sycamore often produces seedlings with three cotyledons. I grew some of these, and they had for a time leaves in whorls of threes, but soon reverted to pairs. I have only heard of one case of an adult wild maple with leaves three in a whorl.

There are some singular mutations which occur in the broad-leaved trees of temperate countries, but probably always under somewhat artificial conditions. They seem to be merely correlated with habit, and are quite independent of affinity. The normal angles at which the branches are set on either become very acute or very obtuse; in the one case we get pyramidal forms like the Lombardy poplar, in the other weeping forms. Or the cell-sap is deeply coloured red, masking the green of the chlorophyll corpuscles (copper-beech). In other cases the leaves lose their normal circumscription, and are deeply divided (fern-leaved beech). Apparently all these mutations are in some degree perpetuated by seed, but they do not hold their own in nature, and owe their preservation to planting in gardens and elsewhere. It is not easy to speculate as to the cause of these singular mutations. One may infer from the fact that they are related to habit, and not to affinity, that they are not due to reversion.

While specific stability under constant conditions appears to be the rule in nature, it is widely different in cultivation. When a plant is brought under cultural conditions it maintains its type for some time unaltered, then gives way and becomes practically plastic. From my experience at Kew, where I saw the process continually going on, I hazarded the generalisation that any species, annually reproduced from seed, could be broken down in about five years. During that period specific stability, though menaced, tends to maintain itself. Darwin was well aware of this, and as for the moment his books seem to be little studied, I will quote the passage:—

"We have good grounds for believing that the influence of changed conditions accumulates, so that no effect is produced on a species until it has been exposed during several generations to continual cultivation or domestication. Universal experience shows us that when new flowers are first introduced into our gardens they do not vary; but ultimately all, with the rarest exceptions, vary to a greater or less extent" ("Animals and Plants," ii., p. 261).

He quotes in support the testimony of well-known experts. Thus Salter, the great raiser of chrysanthemums, states:—"Everyone knows that the chief difficulty is in breaking through the original colour and form of the species." Vilmorin, the most distinguished French horticulturist of his time, maintained that "the first step is to get the plant to vary in any manner whatever; for the fixed character of the species being once broken, the desired variation will sooner or later appear."

Abundant illustrations of the fact which has been stated are furnished by the history of individual species. Thus Sabine says of the first dahlia plants introduced into Europe:—"At Madrid they were a long time in the Royal Garden without any indications of change." The history of the Zinnia and of the Swan River daisy is the same. A few years ago Mr. Watson, the curator of Kew, gave me notes of cases which had come under his own observation. *Primula japonica* was introduced in 1871 as "a new crimson primrose." In 1877 it produced rose-coloured as well as various shades of magenta flowers. *Anthurium scherzerianum*, introduced in 1862, had in 1874 produced enormously developed spathes, and in 1880 a form with two spathes to each spadix. In 1874 it was crossed with a white variety, and that called *rothschildianum*, with mottled spathes, was the result. It is interesting to note that "the seedlings are nearly all mottled like the parent." *Impatiens Sultani* was introduced at Kew from Zanzibar in 1881. For several years it did not vary. It now exhibits half-a-dozen distinct shades of red in the flowers. The fine Cape orchid, *Disa grandiflora*, has long been in cultivation in this country; varieties were unknown until it was raised from seed by Dr. Moore at Glasnevin.

I may add two cases which have more particularly come under my own notice. *Nemesia strumosa*, a rather local South African plant, was introduced about 1892. It now shows indications of breaking up into two distinct races, with the greatest variety in the coloration of the flowers.

The most remarkable, however, is that of *Primula obconica*, introduced in 1882. For many years it was perfectly stable, but within the last few has exhibited the most surprising variation both in the number, form, and colour of the flowers.

In all these cases I think we may safely infer from the persistent specific stability at the commencement of cultivation that the changes which subsequently occur would not have occurred in nature. We cannot regard the one state as in any way a continuation of the other. The changes which occur under cultural conditions are, in fact, something *sui generis*. We may say, of course, if we like, that the potentiality of such changes was latent in the species in nature; but, as a matter of fact, if so, it was suppressed, and there is no evidence of its being called into activity.

The evidence, on the other hand, that such changes follow cultural conditions as a result is simply overwhelming, and I do not suppose that Mr. Lock denies it to be the case. The only question can be as to how they operate. Now there is one point in connection with plants under cultivation the importance of which I have not seen referred to. It is this: in nature we deal with a host of individuals; in cultivation with a very limited number. In my view specific stability is maintained partly by the weeding out of unfavourable variations, partly by wide interbreeding. Now it is obvious that under cultivation the latter agency is inoperative, and cultural conditions bring other influences to bear, especially as regards nutrition; but these I have no space to discuss.

I take it that the species in nature has two weapons in reserve for self-preservation. Of these, variation is the most effective if given time, as it will act automatically. Mutation, on the other hand, though it might now and again hit the mark, is likely in most cases to overshoot it, and is therefore seldom called into play or utilised; and observation seems to confirm this.

In the development of a plant under cultivation the horticulturist takes advantage of both variation and mutation. In the case of the Cyclamen, I have shown that the fine forms now existing are almost entirely the result of accumulated variation. As with the garden Cineraria there has been mutation in floral coloration; but the small amount of variation in the foliage proves, I think conclusively, that in other respects the latter has gradually been evolved from the wild *Cineraria cruenta*, though Darwin, from second-hand information with which I supplied him, stated otherwise.

The case of the Chinese primrose is extremely interesting. When introduced in 1820 it had probably already undergone some cultural development at the hands of the Chinese. It was slowly developed in Europe for a long period without manifesting any striking structural change, until it bore little superficial resemblance to the wild stock. This was discovered in the gorge of the Yangtse, and when introduced into this country proved very intractable of cultivation; in fact, I doubt if at the moment it is to be found in our gardens. Within about the last quarter of a century the cultivated form has exhibited two very remarkable mutations, and it is interesting to observe that these appear to recur independently. The first was the so-called "fern-leaved form." I do not know if this was ever fixed, but, if so, it did not take the public taste, and was not preserved; but fern-leaved individuals appear to occur casually in batches of seedlings of quite distinct races with the ordinary foliage. The transition from the palmatifid to the pinnatifid form of leaf is theoretically interesting. Nothing can well look more different than a fan-palm and a feather-palm, but we can see an intermediate stage in an Australian Livistona. The second mutation, the "ivy-leaved form," is much rarer, and in every way more remarkable. It is apparently accompanied by more or less floral abortion, and it has not been found possible to perpetuate it. The fern-leaved form is, it can hardly be doubted, a reversion to an ancestral type which has been perpetuated in other species, and this may also be the case with the ivy-leaved form, though this is more obscure.

The races of Œnothera which De Vries has raised are nothing more than what a horticulturist would expect; and it may be conceded that if such races could hold their own in nature, distinct species might originate in this way. But there is no evidence that they do; and the probability of their being able to do so is against them. Œnotheras are pretty prolific where they occur, and so far as my experience goes they keep true to type.

Cultural mutations seem, as a matter of fact, to have little, if any, capacity for holding their own in the struggle for existence. I cannot call to mind a single instance of one which has been successful, and even in cultivation there is some reason to think that they are short-lived; but this is a point on which we are in urgent need of carefully ascertained facts. One is told, for example, that new varieties of the potato mostly cease to give satisfactory results after a few years. This is, however, a case of purely vegetative reproduction, and similar statements are made about the sugar-cane, which it is now hoped to regenerate by seminal reproduction. I can remember when potato-fields were covered with flowers and subsequently with fruit. I suppose it was thought antagonistic to tuber-production, as it probably was, and sterile races were selected accordingly. Prof. Hildebrand came to this country to study the subject, and I was able to supply him with information which I had collected for another purpose.

There is practically nothing to add to what has been said on the subject by Asa Gray (" Darwiniana," pp. 338–347). It is notoriously difficult to get hold of old cultivated strains of garden plants, and change of fashion hardly seems sufficient to account for the difficulty. Gray points out " that with high feeding and artificial appliances comes vastly increased liability to disease, which may practically annihilate the race." This has all but happened to the hollyhock, and, left to itself, the Phylloxera would have exterminated the vine in Europe. The existence of a species in nature implies a complicated adjustment to the surroundings. It is not sufficient to launch upon them a new form; in order that it may hold its own, the adjustment must be provided as well. It is by no means always an easy thing to transfer a species from one part of the earth's surface to another. The seed of the Kerguelen cabbage brought back by the *Challenger* germinated freely at Kew, but not a single plant was raised from the seedlings, which all succumbed to a ubiquitous Peronospora.

De Vries has done good service in directing attention to the study of mutations, the nature and origin of which deserve the most attentive study. They graduate into monstrosities which are even more mysterious. It is worth while directing the attention of those who are interested in mutations to the material which exists in Japanese horticultural books. Japanese taste in such matters is widely different from European. In the case of the garden convolvulus (Ipomœa), which is pretty stable with us, the Japanese have figured an extraordinary range of variations which no one else would dream of preserving.

W. T. THISELTON-DYER.

Witcombe, November 9.

The Winding of Rivers in Plains.

BEFORE writing to NATURE on the theory of winding rivers, it would have been wiser for me to have had some observations made as to the conditions of actual flow in the field in different circumstances. It is possible that the more complicated conditions which obtain in some places render the simple theory only partially applicable. My letter was immediately applicable rather to the flow in Prof. James Thomson's simplified model, where the artificial stream had a wooden bed, and the tendency to silt was indicated by short pieces of cotton pinned by one end to the bottom. It may be that the deposit of drift on the inner side of some streams retards their flow by an unexpected amount; and probably there are other causes which prevent the James Thomson theory from being the last word on the subject. I do not pretend to be a field naturalist in any sense, and my cautionary note concerning the flow of glaciers I would ask readers to apply to the flow of rivers also, and to interpret the whole of my letter as a hint and exposition of theory rather than as an assertion and statement of fact.

OLIVER LODGE.

November 20.

SIR OLIVER LODGE's letter in NATURE of November 7 on the winding of rivers in plains has induced me to measure the velocity of flow in different parts of a bend in the river Wey near here. A short line—17 feet—was measured on the bank at the bend, and marks set up at right angles to it, and the time taken by blocks of wood to move between the marks measured with a stop-watch. The distances of the blocks from the inner bank were estimated in terms of the breadth of the river, with the following results:—

Distance from inner bank	Velocity in feet per second
0·3	0·30
0·5	0·45
0·6	0·55
0 8	0·71

This does not bear out his statement that " the flow is most rapid on the inner or sediment-depositing side of the bend," and that the water near the concave bank is nearly stationary, but upholds the common opinion of boating men and others.

The numbers refer to the surface flow only, and it is quite probable that there may be the undercurrent across the bed of the river; in fact, the sudden shelving so often noticed in rivers, and harbour channels where there is a strong tide, has led me to suspect for a long time such cross-currents.

The surface flow-lines are neither parallel nor straight. For this reason a short base line was used, and the velocities obtained are only approximate, but are certainly not far from the above values.

At the end of the experiments two blocks of wood were simultaneously floated down the stream, one near the inner, the other near the outer bank, and the latter won the race by twelve seconds.

However, I noticed that close to the outer bank (within 2 or 3 feet of it) there were back eddies forming a set of feeble whirlpools, and these may play an important part in the scouring.

R. C. SLATER.

Charterhouse, Godalming, November 17.

The Occurrence of Copper and Lithium in Radium-bearing Minerals.

IT is possible that the remarkable action of radium emanation on copper, as recently announced by Sir William Ramsay (NATURE, July 18, vol. lxxvi., p. 269), may not be confined to solutions, but may also occur in the solid state. If so, it should be found that those minerals which contain both radium and copper contain lithium also.

In connection with another investigation, I had separated a sample of pitchblende, from Gilpin County, Colorado, into its principal constituents. The amount of copper in the sample was considerable. The final filtrate, remaining after the separation of the various analytical groups, contained only ammonium and alkali salts. After the evaporation of a portion of this solution, representing about 3 grams of the mineral, and the volatilisation of the ammonium salts, a small residue was left which, when examined spectroscopically, gave a very bright lithium line. This result led me to examine four other samples of uranium-radium minerals. These samples com-